

above the south and north horizons, respectively, of those places, making the considerable corrections needed by the earth's curvature in the 6° of latitude between them, the resulting height of the luminous arch appears to have been about ninety-five miles above a place about fifty miles south of Dingwall. But the times of observation and the altitudes used being only roughly assigned, and only somewhat vaguely comparable together, an uncertainty of, it may be several miles, must no doubt attach itself to this determination.

Two very slender and fugitive streamers only were seen at Slough to rise from the horizon-glow before half-past seven. But between about 7.35 and 7.40 p.m. a dense tuft of them, 8° or 10° wide, rose from a low, faint light-band then visible at about half the altitude of the short arch-segment across  $\zeta$ ,  $\eta$  Ursæ Majoris, crossing and enclosing that wisp of light, to about half as high again as the wisp's altitude from the horizon. This pillar of light grew faintly red before disappearing, which it did in three or four minutes after springing up, while the rest of the glow and the wisps (the one in Hercules being the last one to be seen) also faded away entirely between 7.45 and 8 p.m. The distinct light-band and the tuft of reddish streamers were the only conspicuously bright phases seen at Slough in the aurora of November 23, between 7.15 and 7.45 p.m., with the exception of the two very faint and slender streamers which rose suddenly to a great height across Ursa Major at about 7.30 p.m.

The base of the pillar-like projection, resting on the faint lower light-bow, was at about 8°, and its summit when highest at about 24° above a part of the north-western horizon between 3° or 10°, and 18° or 20° west from north. Its western side would thus be just vertically over Dingwall, if the lower arch which formed its base was at the same time in the zenith of the latitude of Dingwall; and this, it seems quite probable was actually the case, from the following description of the closing scene, by Mr. Pope, of the aurora's progress after the main belt of light had broken up and dispersed itself, at about 7.30. He writes: "At 7.30 p.m. the glow had almost disappeared, while the streamers at this hour were most intensely bright, and appeared to radiate from a small and distinct part of the sky situated in R.A. 358°, Decl. 43° north. Previous to 7.30, no streamers had been observed to radiate towards the south, but between this time and 8 p.m. streamers were seen radiating from the aforesaid part in *all* directions. After 8 p.m. the display gradually faded out, and at 8.30 very little trace of it could be seen. The appearance at the apparent radiant point when the display was at its height was most interesting; appearing sometimes as a solid mass of aurora, then suddenly breaking up into fragments, and assuming most curious forms, uniting again, and so on. Not the slightest trace of the display was visible at 10.30 p.m."

The altitudes of 8° and 24° observed at Slough, at the base and top of the dense column of reddish streamers, nearly in the direction (as described above) of Dingwall, must evidently have related as directly and definitely to this fine display of radiation in the north of Scotland, as the foregoing altitudes at Slough of the bright band of light, before 7.30, seemed clearly and obviously to supply good means for determining approximately the band's real height, by comparing them with Mr. Pope's description of his view of the same band at Dingwall.

Making the same needed correction, as before, for the diminution brought about upon the apparent altitudes observed at Slough by the effect of the earth's curvature, we may thus deduce a resulting real height of 75 miles above the earth's surface, approximately, at the bases, and of 193 miles, approximately, above the earth for the summits of the streamers which clustered over the neighbourhood of Dingwall, and produced the magnificent spectacle of the auroral corona in that part of Scotland.

Should notes have been fortunately preserved at any other towns in Scotland at considerable distances from, and especially in lower northern latitudes than Dingwall, of the aspect, times and bearings, or astronomical positions of this short but bright aurora's rather peculiar features of development in its transient display, much better conclusions regarding the real positions and the extent and distribution of the spectacle might, without doubt, be gathered from them, than those above roughly extracted from only very slight descriptions. But the roughly reached results of the heights of the appearance may yet, for the present, not be entirely worthless, on account of the scarcely doubtful identities of the aurora's bright features, which

were recorded most dissimilarly in this rather surprising instance, at two so very extraordinarily far separated places.

A. S. HERSCHEL.

Observatory House, Slough, December 24, 1894.

#### Peculiarities of Psychical Research.

ON page 200 I see that Prof. Karl Pearson suggests that it would be a good exercise for some one with a strictly logical mind and plenty of leisure to criticise "the products of the chief psychical researchers." May I say, as a member of the S.P.R., speaking for myself and fellow-workers, that we ask nothing better than such a studious and searching criticism. One of our main difficulties is that our critics will not take the trouble to study or even read our evidence, but are content to ridicule what they conjecture to be our methods and results from so great an altitude of assured contempt, that we fail to recognise ourselves in their travesty, and are therefore unable to derive much benefit from their utterances.

Thus, for instance, Prof. Karl Pearson, before writing his recent letters, has evidently not taken the trouble to refer even to the abbreviated summary of certain card-drawing experiments contained in Mr. Podmore's little book; otherwise he could hardly make the statements he does concerning the S.P.R. record of them.

He objects to M. Richet's results as giving insufficient odds in favour of telepathy, so that, as he says, it shows want of acumen to adduce them. Would he then regard it as more scientific to suppress them? On the other hand, the enormous odds against chance, shown in Mr. Gurney's trials, he also says, on page 153, show a want of acumen (I don't know why, but I expect because nothing could possibly exhibit anything else on the part of an S.P.R. worker), and that such odds might be otherwise accounted for. Does he then suppose that the odds of, as he reckons them, two thousand million to three are accepted by us as the odds in favour of telepathy? Probably he does, because there is at present no need to be fair to investigators in an unorthodox field; but Mr. Podmore is careful to state the opposite, as follows (footnote to p. 27 of Mr. Podmore's little summary, in the *Contemporary Science Series*, of the evidence for Thought-transference so far as it exists at the present time): "Of course the statement in the text" [viz. that "the probability for some cause other than chance deduced from this result is '99999998'"] "must not be taken as indicating the belief of Mr. Edgeworth, or the writer, or anyone else, that the above figures demonstrate thought-transference as the cause of the results attained. The results may conceivably have been due to some error of observation or of reporting. But the figures are sufficient to prove, what is here claimed for them, that *some* cause must be sought for the results other than chance." And another quotation may be permitted from Mr. Podmore's preface, which ought to silence irresponsible detractors like Mr. H. G. Wells, and others, who seek to lead the world to suppose that we have some cause at heart other than the simple ascertainment of the truth, whatever it is, and that Mr. Podmore, in particular, is a bigoted upholder of the certainty of telepathy. This is the quotation: "The evidence, of which samples are presented in the following pages, is as yet hardly adequate for the establishment of telepathy as a fact in nature, and leaves much to be desired for the elucidation of the laws under which it operates. Any contribution to the problem . . . will be gladly received. . . ."

Now, I observe that Prof. Karl Pearson has a contribution to the problem, for in *NATURE* of December 27, 1894, p. 200, he refers to certain experiments of his own, wherein by pure chance he obtained results against which the theory of probability also gave large odds. Would he be good enough to let us have these results more precisely, as recorded at the time and signed by witnesses, so that they may furnish us with an example of the methods of a "real scientific investigator"? It will be very unsatisfactory if we have nothing but his memory to rely on for the facts; and as he well knows, it is necessary to have the whole of a large number of trials before deductions from the theory of probability are legitimately applicable.

I observe, finally, that Prof. Pearson, with plenty of good-nature but some lack of originality, has refurbished Dr. Carpenter's old joke about an "ortho-Crookes" and a "pseudo-Crookes," and has directed it against me. I shall be well content if he never manages to find a keener and more effective weapon.

OLIVER J. LODGE.

Liverpool, December 29, 1894.